VOL. 1. NO. 10.

THE Sidereal Messenger.

Conducted by Wm. W. PAYNE,

Director of Carleton College Observatory.

FEBRUARY, 1883.

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"The voice that rolls the stars along,
Speaks all the promises."

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The Sidereal Messenger.

"In the present small treatise I set forth some matters of interest to all observers of natural phenomena to look at and consider."—GALILEO, *Sidereus Nuncius*, 1610.

VOL. 1. FEBRUARY, 1883. NO. 10.

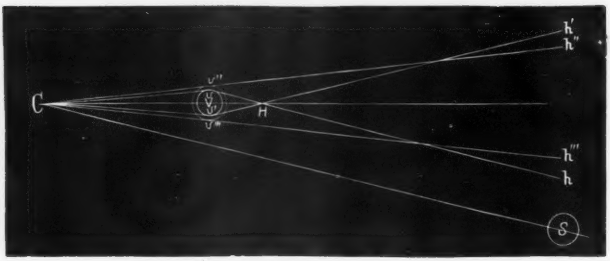
THE PHENOMENA PRESENTED BY THE ATMOSPHERE OF VENUS DURING A TRANSIT.

CHARLES S. HASTINGS.

Mr. J. E. KEELER of Alleghany, Penna., has sent me an admirable drawing of a singular phenomenon observed by him on the 6th of this month at that place, which was also seen independently from the same region by Professor LANGLEY and Mr. BRASHEAR, observing with different instruments. The last named observer has also sent me an interesting sketch of his observation. The phenomenon, briefly described, was a faint patch of light at the limb of *Venus*, exterior to the sun and not symmetrically placed w th respect to the immersed segment of the planet. This patch of light, isolated when first seen at perhaps fifteen minutes before second contact, gradually extended around the planet so that a few minutes later it completed the outline. A probable explanation for this phenomenon and others which will be described in the course of this writing, is the object of the article.

The simplest method to employ in acquiring a definite geometrical conception of the problems involved is perhaps the following: Let C, the place of the observer, be regard-

ed for the moment as a source of light, and let us consider the course only of such rays as would fall upon the atmosphere of *Venus* and not be stopped by the planet. It is clear that such rays would be contained within a hollow cone having C, for an apex and tangent to the two concentric spheres, one the atmosphere of *Venus*, and the other the virtual image of the planet. The diagram shows a meridian section of this double cone. After passing the atmosphere



the whole system of rays is contained within a volume of revolution which is best defined by its meridian section $v'' h''$, $v\,h$, $v'\,h'$, $v'''\,h'''$. One important consideration is readily deduced from the figure. In passing from h'' to h or from h''' to h', the quantity of light received from the side v or v', respectively, constantly diminishes; for if it were constant it would imply that the rays falling between v and v'' must be deflected directly according to their distance below v'', or in other words, that the effect of the atmosphere on a ray of light must increase directly as the depth of its path below the limit of the atmosphere, which is quite contrary to the known laws of variation of atmospheric density. For a similar reason the effect cannot be continually increasing. This conclusion would be true even if $v\,v''$ were not very small compared to Vv, as is assumed, though it could not be established by the reasoning given.

We can now not only determine how *Venus* would appear to an eye placed anywhere within the region just defined, but also, on account of the general principle in optics that the course of a ray is not altered by reversing the direction

of propagation of light, we can state first how the planet would appear if the position of the eye and luminous point were interchanged. Thus if the eye is anywhere within the space v H v', no light from C could be seen; anywhere on the axis of the cone beyond H a luminous ring could be seen concentric with the planet; within the cone h' H h, light might be seen on opposite sides of the planet; outside these two regions again, and within the boundary lines, light from one side only could be seen. If the position of eye and C be changed in any case the appearance is not changed. In the specific case of the effects produced by the sun entering the cone it is necessary to know the dimensions of the various elements indicated in the diagram. Fortunately a famous observation by Professor LYMAN of Yale college yields the data necessary, for he found in 1866 that when *Venus* was 108′ from the nearest limb of the sun a complete ring of light might be seen about it. This makes the horizontal refraction of its atmosphere about 45′, or the total deviation of a ray which first passes the planetary surface $1\frac{1}{2}°$; hence the angle h'' v h is $1\frac{1}{2}°$.

From this it readily follows, since v'' C v''' is only 1′ at inferior conjunction, that the angle v H v', is 3°, and from the known diameter of *Venus*, the distance VH is about 144,000 miles and V S 290,000; but the distance to the sun from the same point is 66,000,000 miles. If then the sun be imagined gradually moving into the space defined by the line h H refracted light may be first seen at the point v; this happens when the angle V C S is about 108′. This light must be very faint, not only on account of the reasoning concerning the varying rate of refraction within the cone given above, but also because such light must have passed very close to the surface of the planet and have lost much by absorption due to suspended vapors and dust. Indeed, Professor LYMAN could only detect this light when his telescope was in the shadow of a cloud or some other distant object.

There is another phenomenon presented by Professor LYMAN'S observations which, though quite unconnected with the matter concerning us here, might readily lead to

confusion, namely, the appearance of *Venus* growing gradually from a crescent of a very little more than 180° compass until its cusps meet. Our reasoning shows that only the light on opposite side from the sun comes to us by refraction, the rest comes by reflection from the body of the planet, and we see the crescent extending because, owing to the planet's atmospheric refraction, more than half its surface is illuminated by the sun. Just because, however, observation shows that this reflected light does not increase in intensity as the two bodies approach, it may be dismissed from farther consideration here as it is quite blotted out by our own atmospheric glare long before the beginning of a transit and thus does not contribute to the phenomena which we are studying.

To return to our diagram. As *S* moves onward in the direction before stated the amount of light refracted at *v* constantly increases by the addition of light which has passed through higher strata, until H *h* becomes tangent to the other limb of the sun, after which all the light refracted from the farther side comes from consecutively more and more elevated strata and consequently grows constantly in intensity. Not until the sun's limb reaches the line $v''h''$, can any light be refracted from the nearer side to the place of the observer at *C*; but this condition corresponds with the epoch of first contact and consequently such refracted light would only appear as slightly increased illumination of the sun immediately adjacent to the planet. This, I think would always escape detection, or at least could not be distinguished from the very strong subjective effect due to the great contrast in brightness between the two bodies, which would be quite similar. The important fact is to be noted that this condition is true of every point of that half of the circumference of the planet nearest the sun, that is, that no point of that half can ever be recognized by refraction. As the sun becomes immersed farther in the cone, the light from the opposite limb not only increases in brightness but also in extent along the circumference of the planet, because the sun, having a large surface, cuts greater and greater portions of our im-

aginary cones. This progression continues until the limb of the sun comes to the axis of the system of cones at which moment there is a remarkable change in the law of increase of brightness in the atmospheric arc. Up to this moment the light has been constantly growing in intensity at the point opposite to the sun and at the same time extending on both sides towards the cusps of the interrupted limb of the sun; at this instant the light from the most distant region comes from all portions of the sun near the plane of the diagram, with a mean deviation of 16′ and a minimum deviation of ½ minute. The light from the extremities of the planetary diameter tangent to the sun's limb comes only from the single point of the sun which is in line with the centre of the planet. A moment later the light from that portion of the atmospheric ring adjacent the solar limb comes from all portions of the sun near the chord joining the cusps; but, as is evident, this comes with small deviation and consequent brilliancy. Hence, although the region of the limb remote from the sun is still increasing regularly in brightness, that close to the sun has taken a sudden increment in brilliancy, which must grow until it quite outstrips that of the former reigion.

We are now prepared to trace all the changes which a telescopic view of *Venus* as she approaches the sun in inferior conjunction would present us. First, the crescent would grow gradually more slender, its horns extending more and more beyond the limits of a diameter until, when the planet is separated little more than 1° from the sun, the cusps would meet and form a ring, thicker, however, on the sunward side. This is the phenomenon observed and described by Professor Lyman. A little later, at a time depending on atmospheric conditions, the planet would totally vanish, in the intense glare surrounding the sun. The next view would be that of the edge more distant from the sun, which would be outlined by an arc of light increasing in brightness and extent with increasing nearness. This the Pittsburgh observers alone seem to have seen, as the observation would demand a purity of sky which

was little likely to occur at any place in the United States. One characteristic feature, however, has not been touched upon in our explanation, namely, the non-symmetrical character of the phenomenon; but we find a suggestion of the cause in the demonstrated fact that this light must have passed through relatively low regions of the planetary atmosphere, hence, if there were any local differences in transparency due to cloud formations etc., such a non-symmetry must have resulted. Indeed, if we are guided by analogies drawn from our own atmosphere, we must conclude that such a want of symmetry is more probable than not.

After the centre of the planet has reached the limb of the sun there would result a rapidly growing intensity of illumination of the atmospheric ring near the cusps, which would speedily outstrip the brilliancy of the more distant portion. This phenomenon, which was doubtless seen by most observers much as it was by the writer, I caught just $4^m\ 40^s$ before second contact, through rather a milky sky, as two slender horns of light shooting out from the solar cusps. One minute and twenty five seconds later they had met and after that there was a continuous growth in brightness. The phenomena observed after third contact was exactly the same, even the noted times being closely accordant.

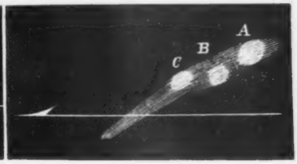
Johns Hopkins University, Dec. 23, 1882.

FIGURE OF THE NUCLEUS OF THE BRIGHT COMET OF 1882. (GOULD).

EDWARD S. HOLDEN.

Although this comet presented a beautiful spectacle, when seen with the naked eye, I have been disappointed at the small amount of work which I have been able to do in the way of accurate observation. I give herewith the only two good sketches which I have been able to make. The

aperture employed was 15 inches and the power was 145 diameters.



1882, Oct. 13. 1882, Oct. 17.

1882, October 13. (See the figure.) The nucleus is curved as in the drawing. It consists of three masses. I am sure of a break at *a*; tolerably sure of a break at *b*, and I suspect a break at *c*, but I am not certain of it.

1882, October 14. The night is very poor. (In general the appearances of last night are confirmed.) The nucleus is about 1′ long.

1882, October 17. (See the figure.) There are three masses, plainly separated. *B* is farther north than the line *A — C* by 3–4″. There is a dark division between each pair of masses. *B* and *C* are nearly in the parallel. The brush of light from the mass *A* toward the east, comes from the south side of *A*, as it is drawn. From the *W* end of *A* to the *E* end of the brush of light, is about 15″.

1882, October 18. The dark space between *A* and *B* is about 10″; it is as wide as *A* itself, and wider than on October 17. *C* is certainly seen as a separate mass; *A* and *B* are brighter and stellar in appearance, more so than on October 17. *C* is, however, fainter than then. The dark axis of the tail extends quite up to the coma.

1882, October 19. Cloudy. The nucleus is seen as before. *A* and *B* are seen, as also the dark space between them. *C* is not seen, but this is probably on account of the unsteady air.

I regret that my opportunity did not allow me to make any further sketches of value.

Washburn Observatory, University of Wisconsin,
Madison, November 3, 1882.

COMET ENVELOPES.

BY E. W. PREBLE.

In the November number of the MESSENGER, there was an interesting article from Prof. HASTINGS on comet Gould, also a figure, representing the nucleus as seen by Prof. YOUNG. From the account and the figure, it appears, that successive envelopes were thrown off from the nucleus, towards the sun, which after a while seemed to meet with a repulsive force sufficient to drive them back in a contrary direction. The luminous matter composing these envelopes, trailing off on all sides, gives the nucleus an oval shape and finally goes to swell the cometary matter of the tail. As Prof. HASTINGS says: "Doubtless the oval form of the nucleus was due to an envelope, which first thrown upwards toward the sun, had already begun to yield to a repulsive force, and may go to swell the splendor of the comet to-morrow." Now, how are we to explain this phenomenon? How is it, that matter, first flying towards the sun, after proceeding some distance, seems to change its course and beat a hasty retreat?

The appearance, is graphically described by Mr. HUGGINS, as follows: "From the glowing nucleus streams flash forth sunward. Shortly their sunward motion is arrested, they gather themselves together, to form one or more bright holes, or envelopes as they are technically called, concentrically arranged in front of the nucleus. Now is seen to take place a change which is most puzzling, namely, these envelopes of light appear to give up their substance under the influence of a strong repulsive force exerted by the sun, and to be forced backward, flying past the nucleus on all sides, still ever expanding and shooting backward until a tail is formed in a direction opposite to the sun." In another place he remarks as follows: "As a comet aproaches the sun, luminous jets issue from the matter of the nucleus on the side exposed to the sun's heat. They are seen to be almost immediately arrested in their motion sunward and to form themselves into a luminous cap; the matter of

this cap then appears to stream out into the tail as if by a violent wind of some kind setting against it."

Through this atmosphere, there is a constant succession of outgoing waves or heat pulses.

Any body of matter, before it reaches the sun's surface, would have to contend with a force, stronger than gravity.

Now, suppose a comet flying into the sun's atmosphere. What would be the result? It would meet the luminous waves as a ship meets the head sea on the ocean. Its course would be retarded more and more as it plowed onward, into larger and longer waves. As Mr. HUGGINS suggests, the lighter matter would be "winnowed out from the other constituents of the cometary mass." If it had an atmosphere of its own, the appearance would be that of light clouds met by a strong opposing wind. It would trail behind like the flags of a steamer running against the wind.

From the comet as from the sun there are outgoing luminous waves. Near the nucleus, these waves would be stronger and longer than the direct waves from the sun, for the comet is itself a heated body, and besides reflecting back the sun's waves, sends out heat pulses of its own. These two combined are for a time, more powerful than the waves they meet, and hence matter is carried out even right against them. Something like this occurs in our own atmosphere. In the summer the sun's rays falling direct upon the earth, heat the surface, and there are strong ascending pulses of heat. At times they are energetic enough to be seen. Something like this occurs in great fires, which proceed against a strong wind. However, at a short distance from the nucleus, the opposing sets of waves would become nearly or quite equal. The sun's would be growing stronger; and the comet's weaker, until neither had any decided advantage. At this *still* place an envelope would be formed by the congregated atoms as drift wood settles in between the equal tide. Such an envelope forced against a resisting medium would be curved precisely as represented. On the same general principle it would be easy to account for the secondary envelope.

ON A METHOD OF PHOTOGRAPHING THE SOLAR CORONA WITHOUT AN ECLIPSE.

WILLIAM HUGGINS, D.C.L., LL.D., F.R.S.

Problems of the highest interest in the physics of our sun are connected, doubtless, with the varying forms which the coronal light is known to assume, but these would seem to admit of solution only on the condition of its being possible to study the corona continuously, and so to be able to confront its changes with the other variable phenomena which the sun presents. "Unless some means be found," says Professor C. A. YOUNG, "for bringing out the structures round the sun which are hidden by the glare of our atmosphere, the progress of our knowledge must be very slow, for the corona is visible only about eight days in a century, in the aggregate, and then only over narrow stripes on the earth's surface, and but from one to five minutes at a time by any one observer."*

The spectroscopic method of viewing the solar prominences fails, because a large part of the coronal light gives a continuous spectrum. The successful photograph of the spectrum of the corona taken in Egypt, with an instrument provided with a slit, under the superintendence of Professor SCHUSTER during the solar eclipse of May 17, 1882, shows that the coronal light as a whole, that is the part which gives a continuous spectrum, as well as the other part of the light which may be resolved into bright lines, is very strong in the region of the spectrum extending from about G to H. It appeared to me, therefore, very probable that by making exclusive use of this portion of the spectrum it might be possible under certain conditions, about to be described, to photograph the corona without an eclipse.

In the years 1866–68 I tried screens of colored glasses and other absorptive media, by which I was able to insolate certain portions of the spectrum, with the hope of seeing directly, without the use of the prism, the solar promi-

* "The Sun," p. 239.

nences.† I was unsuccessful, for the reason that I was not able by any glasses or other media to isolate so very restricted a portion of the spectrum as is represented by a bright line. This cause of unsuitableness of this method for the prominences, which gives bright lines only, recommends it as very promising for the corona. If by screens of colored glass or other absorptive media the region of the spectrum between G and H could be isolated, then the coronal light which is here very strong would have to contend only with a similar range of refrangibility of the light scattered from the terrestrial atmosphere. It appeared to me by no means improbable that under these conditions the the corona would be able so far to hold its own against the atmospheric glare, that the parts of the sky immediately about the sun where the corona was present would be in a sensible degree brighter than the adjoining parts where the atmospheric light alone was present. It was obvious, however, that in our climate and low down on the earth's surface, even with the aid of suitable screens, the addition of the coronal light behind would be able to increase but in a very small degree the illumination of the sky at those places where it was present. There was also a serious drawback from the circumstance that although this region of the spectrum falls just within range of vision, the sensitiveness of the eye for very small differences of illumination in this region near its limit of power is much less than in more favorable parts of the spectrum; at least such is the case with my own eyes. There was also another consideration of importance; the corona is an object of very complex form, and full of details depending on small differences of illumination, so that even if it could be glimpsed by the eye, it could scarcely be expected that observations of a sufficiently precise character could be made to permit of the detection of the more ordinary changes which are doubtlessly taking place in it.

These considerations induced me not to attempt eye-observations, but from the first to use photography, which

† "Monthly Notices," vol. xxviii, p. 88, and vol. xxix, p. 4.

possesses extreme sensitiveness in the discrimination of minute differences of illumination, and also the enormous advantage of furnishing from *an instaneous exposure* a permanent record of the most complex forms. I have satisfied myself by some laboratory experiments that under suitable conditions of exposure and developement a photograph plate can be made to record minute differences of illuminations existing in different parts of a bright object, such as a sheet of drawing paper, which are so subtle as to be at the very limit of the power of recognition of a trained eye, and even, as it appeared to me, those which surpass that limit.

My first attempts at photographing the corona were made with photographic lenses, but uncertainty as to the state of correction of their chromatic aberration for this part of the spectrum, as well as some other probable sources of error which I wished to avoid, led me to make use of a reflecting telescope of the Newtonian form. The telescope is by Short, with speculum of 6 inches diameter, and about 3½ feet focal length. A small photogrophic camera was fastened on the side of the telescope tube, and the image of the sun after reflection by the small plane speculum was brought to focus on the ground glass. The absorptive media were placed immediately in front of the sensitive film as in that position they would produce the least optical disturbance. Before the end of the telescope was fixed a shutter of adjustable rapidity which reduced the aperture to 2 inches. This was connected with the telescope tube by a short tube of black velvet for the purpose of preventing vibrations from the moving shutter reaching the telescope. On account of the shortness of the exposures it was not necessary to give motion to the telescope.

It was now necessary to find the absorptive medium which would limit the light received by the plate to that portion of the spectrum from about G to H. There is a violet (pot) glass made, which practically does this. I had a number of pieces of this glass ground and polished on the surfaces. Three or four of these could be used together, castor-oil being placed between the pieces to diminish the

reflection of light at their surfaces. Some inconvenience was found from small imperfections within the glass, and it would be desirable in any future experiments to have a large supply of this glass, from which more perfect pieces might be selected.

In my late experiments I used a strong and newly made solution of potassic permanganate, in a glass cell with carefully polished sides. This may be considered as restricting the light to the desired range of wave-length, since light transmitted by this substance in the less refrangible parts of the spectrum does not affect the photographic plates.

Different times of exposure were given, from so short an exposure that the sun itself was rightly exposed, to much more prolonged exposures, in which not only the sun itself was photographically reversed, but also the part of the plates extending for a little distance from the sun's limb.

Gelatine plates were used, which were backed with a solution of asphaltum in benzole.

After some trials I satisfied myself that an appearance peculiarly coronal in its outline and character was to be seen in all the plates. I was, however, very desirous of trying some modifications of the method described with the hope of obtaining a photographic image of the corona of greater distinctness, in consequence of being in more marked contrast with the atmospheric illumination.

Our climate is very unpropitious for such observations, as very few intervals, even of short duration, occur in which the atmospheric glare immediately about the sun is not very great. Under these circumstances I think it is advisable to describe the results I have obtained without further delay.

The investigation was commenced at the end of May this year, and the photographs were obtained between June and September 28th.

The plates which were successful are twenty in number. In all these the coronal form appears to be present. This appearance does not consist simply of increased photographic action immediately about the sun, but of distinct coronal

forms and rays admitting in the best plates of measurment and drawing from them. This agreement in plates taken on different days with different absorptive media interposed, and with the sun in different parts of the field, together with other necessary precautions observed, makes it evident that we have not to do with any instrumental effect.

The plates taken with very short exposures show the inner corona only, but its outline can be distinctly traced when the plates are examined under suitable illumination. When the exposure was increased, the inner corona is lost in the outer corona, which shows distinctly curved rays and rifts peculiar to it.

In the plates which were exposed for a longer time, not only the sun but the corona also is photographically reversed, and in these plates, having the appearance of a positive, the white reversed portion of the corona is more readily distinguished and followed in its irregularly sinuous outline than is the case in those where the sun only is reversed, and the corona appears, as in a negative, dark.

Professor STOKES was kind enough to allow me to send the originals to Cambridge for his examination, and I have his permission to give the following words from a letter I received from him: "The appearance is certainly very corona-like, and I am disposed to think it probable that it is really due to the corona." [Professor STOKE'S opinion was formed from the appearance on the plates alone, and without any knowledge of their orientation, and without the means of comparing them with the eclipse plates of May 17.]

I have since been allowed, through the kindness of Captain Abney, to compare my plates with those taken of the corona in Egypt during the eclipse of May last. Though the corona is undergoing doubtless continual changes, there is reason to believe that the main features would not have suffered much alteration between May 17th and September 28th, when the last of my plates were taken. This comparison seems to leave no doubt that the object photographed on my plate is the corona. The more prominent features of the outer corona, correspond in form and general

orientation, and the inner corona, which is more uniform in height and definite in outline, is also very similar in my plates to its appearance in those taken during the eclipse.

Measures of the average height of the outer and the inner corona in relation to the diameter of the sun's image are the same in the eclipse plates as they are in my plates taken here.

There remains little doubt that by the method described in this paper, under better conditions of climate, and especially at considerable elevations, the corona may be successfully photographed from day to day with a definiteness which would allow of the study of the changes which are doubtless always going on in it. By an adjustment of the times of the exposure, the inner or the outer corona could be obtained as might be desired. It may be that by a somewhat greater restriction of the range of refrangibility of the light which is allowed to reach the plate, a still better result may be obtained.

Plates might be prepared sensitive to a limited range of light, but the rapid falling off of the coronal light about H would make it undesirable to endeavor to do without an absorptive screen. Lenses properly corrected might be employed, but my experience shows that excessive caution would have to be taken in respect of the absolute cleanness of the surfaces and of some other points. There might be some advantage in intercepting the direct light of the sun itself by placing an opaque disk of the size of the sun's image upon the front surface of the absorptive screen. Though, for the reasons stated above, I did not attempt eye-observations, there is no reason why with suitable screen, and under suitable atmospheric condition the corona should not be seen directly. I regret that the very few occasions on which it has been possible to observe the sun has put it out of my power to make further experiments in these and some other obvious directions.

Postscript.—Received December 15, 1882.

[I have Captain Abney's permission to add the following letter this day received from him. "A careful examination

of your series of sun-photographs, taken with absorbing media, convinces me that your claim to having secured photographs of the corona with an uneclipsed sun, is fully established. A comparison of your photographs with those obtained during the eclipse which took place in May last, shows not only that the general features are the same, but also that details, such as rifts and streamers, have the same position and form. If in your case the coronal appearances be due to instrumental causes, I take it that the eclipse photographs are equally untrustworthy, and that my lens and your reflector have the same optical defects. I think that evidence by means of photography of the existence of a corona at all is as clearly shown in the one case as in the other."

DR. C. H. F. PETERS' CELESTIAL CHARTS.

B. D. GILBERT, UTICA, N. Y.

A work is being done at Litchfield observatory, Hamilton college, which will fix the fame of its author, Dr. C. H. F. PETERS, for all time to come. Ever since the year 1860 he has been engaged in making a map and catalogue of the stars down to and including those of the fournteenth magnitude; a map which should be "a sure basis for drawing conclusions with respect to the changes going on in the starry heavens." Previous to the discovery of the telescope there were four catalogues of the stars made that of HIPPARCHUS, 150 years before the Christian era, containing 1,080 stars; the "Almagest" of PTOLEMY, which contained 1,030 stars; another, by ULUGH BEIGH, dating from the 15th centuary, and giving 1,019 stars, and are by TYCHO BRAHE, which had only 1,009 stars, but was determined with greater accuracy than the others. Of the numerous catalogues published since the telescope came into use, that of ARGELANDER is the most extensive, altho' it is merely approximate in accuracy, and does not profess to give the position of the stars with exactness. It includes all the stars down to the ninth magnitude between the pole and two

degrees south of the equator. The number comprised in this list is over three hundred thousand, a number which it is fairly startling to contemplate as having their positions determined by one man.

We come now to the work that has been executed by Dr. Peters, and are prepared to put it in comparison with that of his predecessors. The area of the heavens which he has undertaken to map out is less extensive than theirs, since it does not exceed the declination of thirty degrees on either side of the equator. The work has been done with a 13-inch refractor, which, in a clear sky, shows stars of about the fourteenth magnitude, or five magnitudes smaller than the smallest of Argelander's. Here is at once an immense increase in the number of stars within a given area. And of the difficulties to be encountered we may judge from the fact that Dr. Simon Newcomb states that "of the millions of stars of the tenth magnitude and upwards, hardly one in a thousand can be individually known or catalogued." In order to admit of this increase Dr. Peters has enlarged the scale adopted, so that it furnishes an area exactly nine times that of Argelander's charts. But these determinations are not approximate merely; they are as close and accurate as science can make them. Not only was the position of every star marked out with the greatest care and nicety, but after the proofs of the charts were received they were again compared with the actual condition of the heavens, so that the correctness should be secured beyond any question. The present installment of the work occupies twenty charts, each of which covers 20^{m} in right ascension, and 5° in declination. Each chart also comprises from 2,500 to 3,500 telescopic stars. An average of them all would proably show 3,000 stars in each, or an aggregate of 60,000 for the 20 charts.

It is a curious fact that although Dr. Peters has become famous for his discoveries of asteroids, in no one case has the discovery been premeditated. In every case it has happened merely as an incident of his larger and more important work. While he has been engaged in catalouging the stars, a new one for which he could find no previous data

has "swum into his ken," and upon closer examination it has proven to be a "wanderer," a small brother of one of the big planets which even the ancients designated by that expressive name. Thus, even his incidental work has been of great value to science, and has brought its meed of reputation to himself. But it has been fairly won by years of hard and laborious exertion.

It is due to the author to state that the whole of this work — observations, reductions and drafting — has been done by himself without any assistance, and that it is published at his own expense for gratuitous distribution. The charts were executed by the photo-lithographic process at the establishment of RICHARD PETERS, Washington, D. C., and as specimens of that art they are remarkable for their clearness and distinctness. The whole work is a monument to the learning and the untiring industry of its author, and when completed it will be the most perfect work of the kind in existence, and one which, in all probability, will never again be equaled.

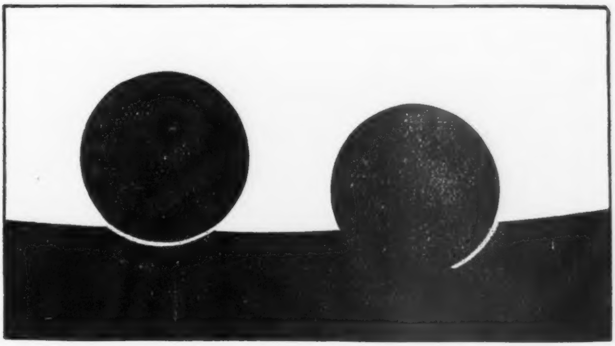
TRANSIT OF VENUS.

E. E. BARNARD, NASHVILLE, TENN.

Dense clouds prevented any observation at first contact. The second contact was uncertain.

At egress when the limbs of the sun and planet were almost in contact, the 'black-drop' formed, through bad definition from heated air from chimney. Contact was estimated through the 'black-drop' and was when the completed outline of the planet and sun were tangent; time 2^h 1^m 45^s, and is probably a little late. A few seconds later, a faint line of light was seen around the protruding limb of *Venus*; this became more noticeable as the planet advanced off the disc, after being visible for several minutes, nearly two-thirds of it disappeared and left a soft, feathery horn of light projecting from the edge of the sun and outlining over one-third, of that part of the planet projecting beyond

the sun. This continued very distinct until $2^h\ 8^m\ 9^s$, disappearing entirely a few seconds later: though closely looked for no such phenomenon was visible at the opposite side of the planet. Possibly dense clouds filled the atmosphere of *Venus* at all other points on the limb save that one, where the atmosphere might have been clear thus transmitting the sun's rays more purely. At fourth contact the definition was better. This contact occured at $2^h\ 22^m\ 15^s$, and can not be but a second or so out. Point of observation was 100 feet west of Vanderbilt observatory. Time was obtained from the observatory by a telegraphic sounder in circuit with the observatory clock and placed beside the observer. The time is Vanderbilt observatory mean time, and as the longitude of the observatory is not accurately known (now in course of determination) the times may be subject to several seconds of error.



I have shown in the diagram two phases of the atmospheric light of *Venus* at egress, these will explain themselves.

Several times during the transit a faint ring of light was seen round *Venus*, this was only certainly seen when thin clouds cut down the bright background of the sun. A peculiar optical phenomenon was seen throughout the transit: the planet appeared of a violet color with a large brownish spot in the middle. This was best seen when the light of

the sun was strongest; by carefully moving the eye so as to look in the eye-piece at a considearble angle the brown spot could be partly thrown off *Venus*, and where it fell on the sun it appeared like a deep yellow stain.

Power for observing contact, 173. Aperture at second contact, full (five inches). Aperture at third and fourth contacts 4.5 inches. A regular sun prism was used with thin dark glass. At second contact no dark glass was used.

Nashville, Tenn. Dec. 20, 1882

THE RING OF LIGHT SURROUNDING VENUS.

J. E. KEELER, ALLEGHANY OBSERVATORY.

At the request of DR. HASTINGS of the Johns Hopkins University, I send to the MESSENGER the following account of a singular appearance presented by the ring of light surrounding the planet *Venus* between the times of first and second contacts, which I observed during the transit of Dec. 5th, but for which I was unable to afford an explanation.

I used a telescope of $2\frac{1}{4}$ inches aperture and 4 feet focus, mounted on an equatorial stand in the open air, and having a positive eye-piece with power of 70, provided with a dark glass of a somewhat greenish tint. A mean time chronometer was placed in a convenient position for noting the time. As soon as first contact occurred, at $20^h 44^m 30^s$, I looked, for indications of the atmospheric ring around the planet, having been requested by Professor LANGLEY to pay particular attention to the physical phenomena of the transit, but for the first three or four minutes could see nothing of it. Shortly afterwards, however, I caught a feeble glimmer of light, almost star-like in appearance, on the limb of the planet farthest from the sun, which at $20^h 49^m$ presented the appearance of a curved streak of very faint silvery light extending for a short distance along the margin of the unimmersed portion of the planet's disc. The brightest part of this luminous arc was not directly opposite the sun, but was

situated about 20° to the west of a line joining the centres of the sun and *Venus*. At the same time little horns of light, due perhaps to an optical illusion, appeared to rise from the cusps of the sun at the margin of the planet, like the elevated rim of fluid which surrounds an immersed body through capillary action.

As the planet advanced, the arc of light gradually extended and brightened, until at 20^h 54^m, or nearly half way between first and second contacts, the unimmersed portion was completly surrounded by a luminous ring. The light at the place on the margin when it was first noticed, however, much exceeded in brillancy that of the adjacent portions, the brightest part extending along perhaps 30° of the planet's circumference, and on the western side the luminosity was more evident than on the eastern, when it was as yet barely discernible. The juncture of the luminous arc first observed with the western cusp of the sun, to which it lay nearest, occurred before the eastern edge became visible. The marginal patch of light now presented the appearance of a local brightening of a continuous ring of light surrounding the planet, and according to my impression at the time, lay without its contour, although thin clouds which had begun to gather, causing the image in the telescope to "boil," rendered a definite conclusion difficult. An independent drawing by Mr. BRASHEAR with a reflector of about six-inch aperture, represents this spot of light as extending within the planet. At 20^h 57.5^m the appearance was still marked, and the ring of light quite brilliant all around the planet. After this my attention was withdrawn from it in preparing to observe the second contact through the fast thickening clouds. At emersion the sky was completely overcast, and observation was impossible.

The phenomenon I have described was also observed by Professor LANGLEY with the large equatorial of the observatory, temporarily reduced to six inchs aperture, and by

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* See page 263 of last MESSENGER.

Mr. BRASHEAR, of Pittsburgh, with a Newtonian reflector. Prof. LANGLEY estimated the centre of the brightest portion to be 30° from the line passing through the centres, toward the west, giving a position angle 10° greater than that recorded by me at the time. As the position angle of the planet on the sun when half immersed was about 148°, the mean of these two independent estimates gives a position angle of 173° for the brightest part of the spot on the planet.

VENUS IN TRANSIT, DEC. 5, 1882.

Allegheny Mean Time $20^h\ 49^m$. 4.5^m after 1st contact. Allegheny Mean Time $20^h\ 54^m$. 9.5^m after 1st contact.

The appearance of *Venus* after the spot was first observed, and at $20^h\ 54^m$, when nearly half immersed, is represented in the accompanying drawing.

Allegheny observatory, Dec. 27, 1882.

TRANSIT OF VENUS.

E. L. LARKIN, NEW WINDSOR OBSERVATORY.

At this observatory, *Venus* was seen in transit until it advanced 30° on the sun, when a great storm of snow and wind burst upon us from the north-west. Through the kindness of the Sup't of the U. S. N. observatory at Washington, and the officers of the Western Union telegraph company, we received time signals direct from the mean

solar clock in Washington at mean noon on Dec. 5, 6 and 7. Time was transmitted by means of a formula devised and executed by the American Transit-of-Venus Commission. The plan consisted of sending pendulum beats by direct wire to receiving stations begining at $11^h 56^m 30^s$ Washington mean time, with break-seconds at beginnging of minutes and half-minutes. Here is a record of the time received at this observatory, as checked from the receiving magnet; and corresponding New Windsor mean solar time.

Washington Time as received.			New Windsor Time as checked.		
h	m	s	h	m	s
11	56	30.	11	2	37.
11	57.		11	3	7.
11	57	30.	11	3	37.
11	58.		11	4	7.
11	58	30.	11	4	37.
11	59.		11	5	7.
11	59	30.	11	5	37.
12	00	00.	11	6	7.

Difference in time $53^m 53^s$, whence this village is in longitude $53^m 53^s$ west. Latitude $+ 41° 13'$.

The beats of the Washington pendulum were distinctly heard and regular breaks carefully noted. At three minutes before the computed time of begining of transit for this longitude, a large cloud passed over the sun, but moved away only one minute forty-one seconds before first contact. This cloud we are sorry to relate was succeded by a mass of invisible vapor, which, however, was sufficient to cause the solar limb to present an appearance of a boiling, turbulent fire, preventing accurate determination of time of contacts 1 and 2. We set telescope on *Venus* by Ephemeris five minutes before calculated time of contact 1, and moved glass up to the edge of the sun so we would loose no time in adjustment. At one minute before the expected time we began close search with power 150, striving to detect the first faint indentation. Soon a fine black line was seen on the south eastern limb and the transit was in progress. At the instant the "boiling" was active, and with all possible care we were unable to decide on the time of contact 1 to within a probable error of three seconds. We viewed the transit with a six-inch Clark Equatorial with

diagonal solar eye-piece, which projects at right angles to the axis of collimation. On each side of this projecting piece was attached a carefully adjusted watch, one reading mean Washington, and the other mean local time. At the moment of contact 1 we took readings, the local time watch indicating Dec. 5, 20^h 1^m 41^s, and Washington time watch, Dec. 5, 20^h 55^m 34^s.

When *Venus* had passed half way on the solar periphery the turbulence nearly ceased for about six minutes, vision becoming quite distinct, and we were gratified to see a clear cut arc of light about the external curve of the planet. It was caused by the refraction of the sun's rays by the atmosphere of *Venus*, and presented a spectacle never to be forgotten. It formed a beautiful semi-circle of light in projection from the edge of the sun. More "boiling" then came on and kept up until contact 2 was passed: we having almost to guess at the actual time, which as we could determine was for local: 20^h 22^m 20^s; and for Washington: 21^h 16^m 13^s, Dec. 5. For 25 minutes the upheaval was incessant, and then quieted down a short time, which was well improved in a close search for a haze or halo about *Venus* derived from its atmosphere, but we failed to detect it, the seeing not proving first-class however. A lookout was maintained until 21^h 5^m local time when an enormous cloud bank obscured the sun, putting out from our vision for all time, after a view of 1^h 4^m, the sublime scene—the transit of *Venus*. A furious storm set in, a frigid wave swept over Illinois, and thermometers went down to 13° below zero.

We did not see the "black drop," a phenomenon on record as having been observed at transits. We are pained to say that all our labors, and the trouble and care of the telegraph company, so far as this observatory is concerned, will have to be set down as useless to the cause of science, owing to the large probable error of 3 seconds in time of contacts.

A SMALL ASTRONOMICAL LIBRARY.

The following list of books was made out and sent to various book dealers with a request for an estimate of the cost. Below we give the estimate furnished by B. WESTERMANN & Co., of New York City. This list may be of use to amateur astronomers and to astronomical societies here and there. The excellent list of books given at the end of the German translation of NEWCOMB'S Popular Astronomy should also be consulted.

Neison, The Moon, (cloth)..$8.50

Clarke, Geodesy, (cloth)	3.38
Lockyer, Star Gazing, (cloth)	5.67
Grant, History of Physical Astronomy, (cloth) 1852	4.32
Gauss, Theoria Motus (English) 1857	4.00
Proctor, Saturn and its System 1865	3.78
Chambers' Descriptive Astronomy 4877	3.24
Nature, Vol. 1-22 (1880) if obtainable now, unbound	77.50
Observatory, 1877-80	12.96
Subscription 1883	3.25
Baily's, Catalogue of Lalande's Stars 1874	9.60
Baily and Henderson's Lacaille's catalogue of 9766 stars	3 65
Radcliffe Catalogue, 1st part 1847	?
" " 2nd part. 1870	4.05
Melbourne, General Catalogue 1870	?
Cape, Catalogue of 1159 Stars 1860	1.35
Barlow's Tables of Squares, 1875	1.62
Everett, Units and Physical Constants	1.22
Secchi, Le Soleil, 2 vols, and atlas	9.00
Flammarion, Catalogue des etoiles doubles	2.00
Pontecoulant, Theorie Analyt du Syst du Monde 1856 vol. I, II.	4.50
" " " 3 vols. 1829-37	3.60
Brunnow, Traite d'Astronomie, 2 vols (Lucas & Andre) 1869-72	5.00
Lagrange, Mecanique, 3d edition, 1855	10.00
Poisson, Traite de Mecanique, 1853	4.50
Albrecht, Formeln & Hulfstafeln, 1873	2.34
Oppolzer, Lehrbuch d'Bahnbest, I. II	11.96
Bessel, Abhandlungen, 3 vols	15.08
Encke's Abhandlungen, 3 vols	7.80
Olbers, Methode des Bahn eines Cometen zu Berechnen, 1867	2.15
Klinkerfues, Theoret. Astrom	2.34
Mobius, Mechanik des Himmels	1.56
Oeltzen's Argelander's Zonen Beob von−15 bis—31° 1 vol. 1857-8 (scarce)	7.00
Lamont, Verzeichniss, von 3571 sternen 1871	1.65
" " 6323 " 1869	1.35
" " 4793 " 1869	1.04
" " 4093 " 1872	1.35
" " 9412 " 1866	2.40
" " 5563 " 1874	1.80
Weisse, Positiones mediæ etc., + 15°, + 45° 1863	3.00
Same—15° to + 15° 1846 (scarce)	31.00
Houzeau: Vade Mecum de l'Astronome (1882)	?
Chauvenet, Astronomy	6.00
Watson, "	8.50
Gould, Uranometria and atlas	?

EDITORIAL NOTES.

This number completes the first volume of the "MESSENGER" according to the general plan of publication, that each volume shall consist of ten numbers, issued monthly, during the year except for July and September. The first three numbers contained twenty-four pages each; to subsequent ones eight or ten pages have been added, nearly all of which have been set in smaller type. It seemed desirable to enlarge the "MESSENGER" because of the general interest in the publication, the amount of excellent matter constantly coming to hand, and, more than all, the real need of it in order to follow at all faithfully the ambitious endeavors and successes of American students in astronomy.

The substantial aid and encouragement received during the year, from many leading astronomers in this country has been as unexpected as it has been gratifying; and it may be said in this connection, that our excellent friend, Professor E. S. HOLDEN of Washburn Observatory, Wis., has contributed to this enterprise largely and constantly from the first, so that it is but just to him to say that the "MESSENGER" would have been less and different but for his own ready and efficient help.

In view of a limited support which was to be expected during the first year, only a small outlay could be devoted to engraving for needed illustration. In this, we have been encouraged in the past, and it will be continued. If we are successful in maturing plans already favorably under way, we will be able soon to present brief sketches of all the principal American observatories with suitable illustrations of the best astronomical instruments now in use in the various departments of the science. Some knowledge of the different kinds of work now being done by leading astronomers, and the methods they use ought to be helpful to the amateur observer, and useful to teachers of astronomy everywhere. Our facilities for obtaining such information are better than ever before, and as soon as our patronage will warrant it, the "MESSENGER" shall be improved in several ways.

All single subscriptions which began with number one of the first volume expire with this number. Subscribers are respectfully requested to notify us early if they wish the "MESSENGER" continued, otherwise it will be assumed that they prefer to have it stopped.

The leading article of this issue, by DR. C S. HASTINGS of Johns Hopkins University, Baltimore, and other brief ones by J. E. KEELER, of Alleghany observatory, Pa., and E. E. BARNARD, of Nashville, Tenn., deserve special perusal, because the latter present valuable observations, and the former offers an explanation of curious and puzzling

phenomena attendant upon the late transit of *Venus*. One error was noticed in the copy of Dr HASTINGS article too late for correction. At the intersection of lines $v'' h''$ and $v''' h'$, s should be placed; then in line 16 from bottom, page 275, *VS* should read *Vs*.

Observations connected with the transit of *Venus* were more or less successful, as shown in the appended list of stations, which are all that have been thus far heard from. This condensed way of presenting the work was suggested by the New York Times.

The figures 1, 2, 3, 4 denote that the corresponding contacts were observed; *P* denotes that photographs were made on the same plan as those of the Government parties; *P**, photographs on some different plan; *h*. heliometer measures; *h**, measures for the same object as heliometer measures, but made with a different instrument more or less completely equivalent; *s*, spectroscopic observations; *p*, photometric observations; *m*, micrometer measures of the planet's diameter:

Ottawa, Canada—1, 2, 3, 4.—Kingston, Canada—2, 3, 4.—Cambridge, Mass.—1, 2, 3, 4, *s*, *p*, *m*; several observers.—Providence, R. I.—2, *P**, (23).—Amherst, Mass.—3, 4. - South Hadley, Mass.—3, 4, *s*.—Hartford, Conn.—2, 3, 4, *h*, *m*; German party.—New Haven, Conn.—1, 2, 3, 4, *P**, 150), *h*, *m*; several observers.—Helderberg Mountain, N. Y. — 1, 2 — West Point, N. Y.—1, 2, 3, 4.—Poughkeepsie,—N. Y. 3, 4, *P**, (9).—Brooklyn, N. Y.—1, 2, 3.—Columbia College, New York City—2, 3, 4. Western Union Building, New York City—1, 2, 3, 4.—University City of New York, New York City—1, 2, 3, 4.—Elizabeth, N. J.—2, 3, 4.—Princeton, N. J.—1, 2, 3, 4, *P*, (188), *s*, *m*; several observers.—Philadelphia Penn.—1, 2, 3,4.—Easton, Penn.—1, 2,3,4.—Alleghany, Penn.—1, 2, (?) *s*, *m*.—Pittsburgh, Penn.—2, 3.—Wilmington Del —1, 2.—Baltimore, Md.—2, 3, 4; several observers.—Annapolis, Md. 2, 3, 4.—Naval Observatory, Washington, D. C.— 1, 2, 3, 4: *P* (53), *m* several observers.—Coast Survey, Washington, D. C.—2, 3, 4; several observers.—Signal Service, Washington, D. C.—1, 2, 3, 4.—Charlottesville, Va.—2. 3, 4.—Aiken, S. C—3, 4, *h*, *m*; German party.—St. Augustine, Fla.—1, 2, 3, 4 *h**, *P**, (200), *m*; French party.—Cedar Keys, Fla.—2, 3, 4, *P*, (180), *m*; Government Party.—Chicago, Ill—1, 2; several observers —Madison, Wis.—1, 2.—Northfield, Minn.—3, *m*.—Iowa City, Iowa—1, 2.—Ann Arbor, Mich.—4, *m*.—Tarrytown, N. Y.—1, 2, 3, 4.—Geneva, N. Y.—3,4.—Columbus, O.—3, 4.—Phelps, N. Y. 2.—Cleveland O. 4.—Iowa City, Ia.—1, 2, 3 —Haverford, Pa.—1, 2, 3, 4.—Baltimore, Md. 1, 2, 3, 4;—several observers.—South Bethlehem, Pa.—1, 2, 3, 4.—New Windsor, Ill.— 1, 2.—Marietta, O.—2.—Greenville, S. C. (Furman University), 1, 2, 3, 4.—San Antonio, Texas—3, 4, *P*, (204), *m*, Government party.—San Antonio, Texas—3, 4, *h**, *m*; Belgian party.—Fort Selden, New Mexico—1, 2, 3, 4, *P*, (216), *m*; Government party.—Lick Observatory, California—3, 4, *P*, (147), *m*.

FOREIGN.

Potsdam, Prussia– 1, 2, *P**, *s*, *m*.—Jamaica--1, 2, 3, 4.—Puebla, Mexico–1, 2, 3, 4, *h**; French party.—Chapultepec, Mexico– No contacts; *P*†, (13). Cape Town, South Africa—1, 2, *P*; (?) American Government party. Durban South Africa–1, 2.–Tasmania 3, 4, *P*; (?) American Government party—Melbourne, Australia—3, 4, *P**, (33).–New Zeland–3, 4, *P*; (236) American Government party.—Santiago, Chili—Completely successful; *P*; (?) American Government party.- Santiago, Chili- Completely successful; *h** Bergian party.

Our readers will surely be gratified to see the full and plainly worded statement of Professor C. A. YOUNG of Princeton, respecting the work done during the late transit of *Venus*. We have added several pages to the usual size of the MESSENGER to present this excellent review in its entirety.

"On the whole the observations of the transit have been successful beyond expectation. Although in the United States there was more or less cloudiness, yet there were very few stations which did not succeed in accomplishing the most essential portions of their intended work.

The observations divide themselves broadly into two great classes; those—the most elaborately prepared for—which aim at data for determining the solar parallax*, and those which relate to the planet itself—observations spetroscopic, photometric and micrometric.

The first class embraces observations of "the contacts;" measurments, (with the heliometer or other equivalent instrument,) of the distance of the planet from the sun's edge at intervals during the transit; and photography of the sun's disk with the planet upon it.

CONTACTS.—First, then, as to contact observations Since *Venus* is much less than a third of the way from the earth to the sun when she passes between us, every one can see that the moment when she reaches the edge of the sun's disk it must vary with the observer's station, being earlier in some places than in others. Now, the relations of things are such that if we could get *absolutely accurate* observations of these contacts at half a dozen widely separated stations, we could compute not only the slight deviations of the planet from its predicted position, but also its precise diameter, and the *parallax and distance of the sun*. The difficulty is in making the contact observations sufficiently precise; and in the presnt condition of optical art this difficulty lies mainly in the fact that the planet is surrounded with an atmosphere, which makes the edge indistinct, and renders it doubtful just where the planet's disk begins and ends. With any but excellent telescopes and eyes

*The sun's parallax is equal to the semi-diameter of the earth as seen from the sun. When we say that the parallax is 8.8″ it is the same as saying that, seen from the sun, the earth's diameter would be twice that, i. e., 17.6″. Knowing this and the size of this earth the sun's distance follows at once.

there are, moreover, other difficulties arising from what is called irradiation; but these can mainly be overcome by practice with an "artificial transit." This is an apparatus by means of which a fictitious planet is drawn by clock-work across a fictitious sun, the dimensions and distance of the apparatus being such that the appearance presented is as much as possible like the real event. The observer learns how the thing will look, and, after a little experience, comes to seize upon the true movement of contact with much greater accuracy and consistency than at first. But this apparatus does not reach the difficulty due to the planet's atmosphere, and no method yet proposed will do it.

On some accounts the first contact is the most difficult of observations, since usually the planet cannot be seen until after it has begun to encroach upon the sun. I say *usually* because when the sky is very clear and the telescope and observer both good, the planet's disk may sometimes be perceived before it touches the sun at all. In 1874 JANSSEN thus saw *Venus*, and in 1878 LANGLEY saw *Mercury* under similar circumstances. Probably the ring of light produced by the refraction of the planet's atmosphere is aided by the background of the sun's corona to produce this effect. But such good fortune seldom comes and, so far as we can learn, happened to none of the observers on Dec. 6. It has been common to regard this contact as having less value than the others, because of this difficulty of observation, but all the more recent experience of the later transits of *Mercury* and *Venus* goes to show that when proper care is taken to know the precise point on the sun's limb where the contact is to take place it is just as good as either of the others. It is, however, a little more likely to be lost, and on the day of the transit the weather was such as to enhance the difficulties, so that, in fact, it was not seen by more than half as many observers as the other three contacts.

According to the summary which is elsewhere given it was noted at 20 out of the 39 stations on this continent at which it might be visible; and where there were several observers at the same station it was also generally missed by some of them.

At Princeton, the writer secured an unusually satisfactory observation, using the great telescope with its whole aperture of 23 inches. The polarizing eye-piece employed is of the Merz form, except that the first reflector is replaced with a hollow prism of metal with glass front and back, the cavity being filled with water. The magnifying power was 157. A minute or two before the predicted time the sun's disk came out distinctly through the veil of thin clouds which covered it. Vision was reasonably steady, and the faculæ and granules of the solar surface could be well made out; but the field was full of scattered light, and not the slightest glimpse could be caught of the planet, though most earnestly looked for. The predicted time of first contact by the

sidereal clock (allowing for its known error of 15^s,) was $14^h\ 6^m\ 1^s$. The moment came and still no sign of the planet Ten seconds passed, then 20^s; nothing yet. At 24^s I thought I perceived a little change in the appearance of the sun's limb at the precise point where *Venus* was to strike; each succeeding second strengthened the impression. At 30^s there could be no doubt that something was the matter, though there was yet no visible indication only a sort of hardening of the outline. In a second or two more the notch caused by the advancing planet became evident; at 40^s it was obvious and at 50^s conspicuous. It then extended along an arc of 30° of the planet's circumference,(its chord being about $15''$ or $20''$,) and I ceased looking. I set the contact as occurring at $14^h\ 06^m\ 30^s$ by the clock, which reduced to Washington mean time, gives $8^h\ 55^m\ 40^s$, A. M. The number telegraphed to the papers was six seconds earlier, corresponding to the first glimpse obtained, but the effect then noted was probably due to the planet's advance upon the chromosphere of the sun. This chromosphere, though not bright enough to be seen distinctly, yet probably gives so much light that its obscuration can be noticed, felt rather than seen, by side glances, in the same way that stars, too faint to be visible by straight forward looking, can be caught by averted vision. I am confident that the time of the planet's contact with the outer limb cannot vary more than a second or two from the time stated. One other observer caught it at $6^m\ 38^s$, a third at $6^m\ 49^s$, and a fourth at $6^m\ 50^s$, but he distinctly saw that his observation was late. The rest of the Princeton observers did not perceive the planet for one reason or another until after 7^m, when the notch already had considerable depth.

The difficulties presented by the internal contacts are of a different sort, but not really any less injurious to accuracy. At the late transit the sky so cleared up at most of our Eastern stations soon after the first contact that the ring of light surrounding the planet became visible—brightest (as it ought to be) just at the point most distant from the sun. As seen at Princeton with the great telescope—its aperture being now reduced by a cap to 6 inches—this delicate ring appeared to have a width of about $2''$, and was made up of little filliments standing out from the planet like short, fine hairs close together, with here and there a scintillant knot. We did not see, however, at Princeton the peculiar light on the planet's disk noted by Professor LANGLEY at Pittsburg about the same time.

As the time of contact approached, the sky at Princeton became nearly clear, and the definition was very fine and steady, so that the markings of the solar surface were beautifully distinct, and the halo of the planet's atmosphere was simply exquisite, arching over the dark globe as if the sun were reaching out delicate arms of light to embrace h daughter.

At $14^h\ 26^m\ 50^s$ (by the sidereal clock) I judged that the disk of the

sun would be just tangent to that of the planet if the atmosphere were out of the way; at $14^h\ 27^m$ the atmospheric halo still formed a slight, but perceptible, projection from the sun's limb; at $14^h\ 27^m\ 12.5^s$ it disappeared, leaving the limb quite smooth, and I marked this as the moment of contact, which agrees almost exactly with the mean of the times noted by the other Princeton observers Observers at other stations where the air was favorable had similar experiences, though from the accounts which have appeared so far it would seem that Princeton was rather exceptionally favored in respect to the first and second contacts.

At the third contact the same phenomena should have been repeated in inverse order if the circumstances had remained the same. Probably this was seldom exactly the case. At Princeton, certainly, it was not so. The clouds were rather thick at the critical moment and the seeing a little unsteady, so that nothing was seen of the planet's atmosphere, and the time of contact noted was that when the thin line of light between the planet and the sun vanished; immediately after it the dark edge of the planet's disk began to project beyond the sun's outline. A few minutes after the the third contact it cleared up somewhat, so that rudiments of the atmospheric halo were visible by glimpses, but there was no such revelation of the planet's outline as in the morning, nor was the seeing as steady, so that, as far as the writer, at least, is concerned, the observation of the last contact was hardly as satisfactory as the first.

From the summary it appears that at the 40 stations enumerated (on this continent) the first contact was observed at 20, the second at 29, the third at 32, and the fourth at 30.

So far as can be judged from a hasty and incomplete reduction of the observation at Princeton, it would appear that the planet was about 20^s to 25^s behind time in her orbit, and that her diameter assumed in the computations was at least $1''$, and perhaps $1.5''$, too large. The duration of the transit appears, also, to have been about 25^s longer than computed, which might indicate either of two things or a little of both —that the planet's path was $1''$ or $2''$ of arc north of its computed position, or that the diameter of the sun is a trifle larger than it was assumed. The agreement, however, is remarkably close.

Heliometer and equivalent measurements. - The heliometer observations were made at only a few stations—by German parties at Hartford, Conn., and Aiken, S. C., and by Professor Waldo, at Yale college.

The instrument consists essentially of a telescope, the object-glass of which is divided into two parts which can be made to slide past each other, thus: . The effect is to produce two images of anything looked at, and by properly setting the lenses the image of any object can be made to coincide with any other near it, and then the

reading of a scale attached to the slides which carry the lenses will measure the angular distance between the objects. It is capable of measuring distances much greater than other forms of micrometer can deal with, such, for instance, as the diameter of the sun, whence the name of the instrument. The heliometers used by the German parties were rather small having object glasses only three inches in diameter. The Yale heliometer, the only one owned in the country, is much larger, six inches in aperture, and probably the finest instrument of its class ever constructed

During the transit the observers in charge of these heliometers measured just as rapidly as possible the distances between the two edges of the planet and the nearest and remotest points on the sun's limb, each complete "set" requiring at least sixteen pointings and readings. The Hartford party obtained six full "sets,, and four half-sets, besides a number of measures of the planet's diameter. At Aiken they only obtained three full "sets," with some diameter measures. It is not stated just how the results at New Haven are to be divided. About 260 *readings* are reported, which *might* mean sixteen full "sets," but probably does mean some smaller number of "sets," plus a considerable number of measurments of the diameter of the sun and planet, since such measures are said to have been made.

At St. Augustine the French party did not use the heliometer, but had a double-image micrometer of a different kind devised long ago by the distinguished astronomer, ARAGO, and depending for its action upon the double refraction of Iceland spar. They report themselves as completely successful, though the precise number of measurments is not given.

The Belgian party, at San Antonio, also made measurments of the distance between the planet and the sun's limb, but no statement appears as to the instrument employed. It may have been a heliometer, but probably was not.

Observations were also made at Cambridge and New Haven by noting several hundred times the passage of the planet and sun across a system of fine lines ruled on glass and placed in the focus of the telescope. It would not be at all strange if the results gotten by this simple method should be found to vie in accuracy with those obtained by more complicated forms of apparatus.

PHOTOGRAPHS.—The photographs were made in two different ways. The Government parties at Washington, Cedar Keys, San Antonio, and Fort Selden, and the Princeton and Lick observatories used apparatus of precisely the same construction employed by the American parties in 1874.

The image of the sun is formed by a lens of 5 inches diameter and about 40 feet focal length. As it would be impracticable in the field to point so long an instrument directly toward the sun and keep it so

pointed, it is placed horizontally in a north and south direction, and the sun's rays are directed through it by a flat mirror of unsilvered glass. The mirror, with its clock-work and the lens, is mounted on a pier just in the line of sight of a meridian instrument, and the frame for carrying the sensitive plate is erected on another pier inclosed in a separate light-tight building of its own—the photographic-house. The frame carries a so-called "reticle plate" of glass ruled with fine lines into centimetre squares, and also a plumb-line of fine wire which hangs between the reticle plate and the sensitive surface. Each picture, therefore, has upon it the image of the squares and of the plumb-line.

It is important to know exactly the distance from the lens to the sensitive surface, and a "measuring-rod" of iron pipe is therefore supported by a trestle-work just above the line of sight. A long tube projects from the photographic-house reaching nearly to the lens. The exposure is made by sliding a shutter carrying a slit, about seven inches high and an inch or so wide, across the opening in the side of the house where this tube joins on; and the slide is electrically connected with the chronograph in the observatory, so that an automatic record is obtained of the moment of each exposure. The pictures of the sun are about $4\frac{1}{2}$ inches in diameter, and the diameter of the image of *Venus* is just $\frac{1}{8}$ of an inch. During the transit the exposures were made when the sun was clear at the rate ordinarily of one every minute and a half or two minutes, the intention being to secure about 200 pictures in the five hours between the two internal contacts. No attempt was made to determine the moment of contact by photography, experience having shown in 1874 that the results thus obtained have no value whatever.

During the operation one person attends to the direction and adjustment of the heliostat, so that the image may be properly kept upon the plate; while in the photographic-house three persons are employed—one who places the pictures in the reticle-plate and manipulates the slide, one who hands up the sensitive plate from its box and puts it away after exposure, also keeping the record, and a third who is busy in developing the plates as fast as possible, taking perhaps one in every three or four, and so keeping the party informed as to the success of their operations and any needed changes in duration or exposure.

If all other conditions are favorable and proper care taken, the only anxious point in this method of photography relates to the *flatness of the mirror*. If its surface be curved to any perceptible extent the results will be fallacious; they may be consistent, but they will be untrue, and if the mirror *changes* its curvature during the operation they will be inconsistent as well as untrue.

The writer confesses to considerable misgivings as to this point, fearing the effect of the sun's rays, alternately clear and clouded, upon

the mirror. A little experience the week before the eclipse was anything but reassuring. We had found the light of the reflected image less powerful than might be desired, requiring too long an exposure to suit us, and we concluded to try the effect of *silvering* the surface of the mirror. After silvering the miror we found that the focus of the lens was apparently lengthened *over two feet.* This could be accounted for only in two ways—one was to suppose that the film of silver deposited was about 1-5000 of an inch thicker at the center than at the edges of the mirror, thus giving it a convex form; the other, and far more probable, supposition was that since the film of silver now prevented the passage of the sun's rays into the body of the glass its front surface above became warmed by contact with the film, and thus the mirror was buckled into convexity. Of course, we at once removed the silver, but there remains a fear that even with an unsilvered mirror there may be notable changes of form under the influence of a changing radiation—changes perhaps sufficient to vitiate the results. Time only will show when the work comes to be reduced.

Except at Washington the photographic operations seem to have been unexpectedly successful, as the numbers given in the summary sufficiently show.

At Princeton a considerable number of the 188 were spoiled by clouds, perhaps 20 or 30, and a great many more are inferior to what they would be if the sky had been really clear. At Washington only about 53 were made. At Cedar Keys and San Antonio clouds also interfered to some extent, but at one place 150 and at the other 200 pictures were made. At Fort Selden and at the Lick Observatory the day was perfect, and the photography went on without a hitch.

At a few other places photographs of more or less value were made by other methods. At New Haven about 150 pictures were taken with an 8-inch telescope, giving an image about 1 inch in diameter. Mr. Wilson, who operated the instrument, devised a very ingenious way, which has not yet been described, of getting a horizontal reference line on the plate by means of a level surface of mercury photographed upon it at the same moment with the sun's image. This would seem to dispose of one of the old difficulties of this method of photography, though it does not yet appear how the other (of scale value) will be dealt with. At Poughkeepsie 9 photographs were obtained by some method not explained, and at Providence 23. At St. Augustine the French party took over 200. Probably in both these cases the pictures were made in the same way as at New Haven, though without the horizontal reference line. The French may, however, have employed the same apparatus they used in 1874, which closely resembles the American, but on a smaller scale, the focal length of the lens being only about 14 feet. In 1874 they also used the old daguerreotype process, on metal plates, instead of the collodion film on glass. The Amer-

icans in 1874 used the wet process; this year they employed dry plates, prepared a day or two before the transit by an emulsion process, preferring them to the ordinary dry plates of the market, because of the danger of injury to the latter in passing through tropical climates, as well as for other reasons.

MICROMETRIC OBSERVATIONS.—When *Venus* is upon the sun's disk we have an admirable opportunity of determining her diameter, because at that time she is nearest us; and moreover then, and then only, shows us her true form; at all other times she presents a phase like the moon. For this reason all the observers who were provided with suitable apparatus took great pains to measure her diameter as carefully and in as many different ways as possible—by observing the passage across wires or lines ruled on glass, by film micrometers, double-image micrometers, and heliometers—all available instruments were pressed into the service. Observations of this kind seem to have been made at 15 or 16 stations on this continent, and perhaps at nearly as many more foreign stations. The results are not yet reduced, but the indications correspond with the conclusion which was drawn from the contacts—viz., that the planet's diameter is really considerably smaller than hitherto assumed.

THE ATMOSPHERE OF VENUS.—We have already mentioned the beautiful halo of light which surrounded and defined the disk of the planet between the first and second contacts. In connection with this should be noticed the remarkable observation of Professor LANGLEY, who, when the planet was about half entered upon the sun's disk, saw in this halo a bright point or patch of more brilliant light extending along some 20° or 30° of the planet's circumference, and apparently spreading inward over its disk. According to his observatian it was not on the line joining the centres of *Venus* and the sun, but at least 20° south of that line. If it had been on that line there would have been nothing very surprising in it, because it is precisely that point of the planet's atmosphere most remote from the sun's disk which ought, by its refraction, to send us the most light from the sun's surface. In a clear sky, at the moment of external contact, the atmosphere at the point just about to touch the sun would send us no light at all, and the halo surrounding the planet should increase in width and brightness both ways from this place to its opposite maximum. It is perhaps not impossible that some local cloudiness on *Venus* shifted the point of maximum brightness enough to account for the observed asymmetry; or the phenomenon may be due to a very different cause. Several observers have already, at one time and another (see "Webb's Celestial Objects for Common Telescopes," "Chambers's Descriptive Astronomy," and other similar works) reported strange lights seen on the dark portion of the disk of *Venus* when she was crescent formed. Now, remembering what auroral and electric phenomena we are familiar with in our own atmosphere, it is not unlikely that on *Venus* similar

phenomena should be presented on even a more extended scale, since she is nearer to the sun than we, and has, therefore, presumably an intenser meteorology as well as a denser air.

During the transit Professor HARRINGTON, at Ann Arbor, carefully examined the planet's disk and became fully satisfied that he could make out spots and markings on it. So far as we know, however, no one else has reported the same.

PHOTOMETRIC OBSERVATIONS.—At Cammbridge a series of photometric comparisons was made between the disk of *Venus* and the sky just outside the sun's limb. The result was to show that *Venus* was distinctly darker than this part of the sky—as might have been (and was) anticipated, since, at the edge of the sun the illumination of our own atmosphere is reinforced by the light from the chromosphere and corona. One or two observers, at the time when *Venus* was half on the sun, received, much to their surprise, the contrary impression—that the disk of *Venus* inside her atmospheric halo was *brighter* than the background outside. Possibly this singular effect may have been simply subjective, or perhaps due to the fact that the whole circumference of the planet's disk was necessarily illuminated by a powerful twilight. To the writer, with the full aperture of the Princeton telescope, the disk of the planet seemed always, when the air was clear, intensely black, and darker than the background at the sun's limb. It was, perhaps, a little shaded at the edge, but otherwise he could detect no marking or difference of illumination of any kind.

SPECTROSCOPIC OBSERVATIONS. So far as known to the writer, spectroscopic observations were made in this country only at Cambridge, South Hadley, Princeton, and Allegheny. For the most part the results were purely and surprisingly negative, no conspicuous evidence of selective absorption being shown by that portion of the planet's upper atmosphere which alone could transmit to us any light. We were a little more fortunate at Princeton, however in finding distinct indications of water vapor, thus confirming certain old observations of Huggins. At the Halsted observatory a diffraction spectroscope was used first with a grating of 17,000 lines to the inch, and afterward with another one having about 5,700. The slit was placed nearly tangent to the planet's limb, *at the point furthest from the sun's centre,* since there the sunlight would have to traverse the greatest depth of the planetary atmosphere. With neither the highest or lowest dispersion could I detect the slightest effect upon the great "B" group—much to my surprise I own—nor in the "A" group either. But in the high dispersion spectrum the atmospheric water lines between and near the two D's were distinctly and obviously, though not conspicuously, strengthened. I had intended to try a prismatic spectroscope of low dispersion in the hope of detecting in the spectrum diffuse bands which would be most easily seen with such an instrument, but when

I finished with the diffraction spectroscope the clouds had become so thick that there seemed to be very little chance of success. I therefore took off the spectroscope and put on the micrometer, which was not much affected by the clouds so long as the planet's disk was visible at all. At the School of Science Observatory, MR. MCNEILL, with a prismatic spectroscope, (by Grubb,) attached to the $9\frac{1}{2}$-inch equatorial, obtained results entirely accordant with my own. In addition he thought he detected a distinct effect of the planet's atmosphere upon the line at 6,392 of Angstrom's scale, and he suspected one or two other lines.

On Angstrom's map this line, No. 6,392, is ascribed to *iron* which cannot very well be supposed to exist as vapor in the planet's air; but it is quite possible that the line is really composite, and that one of its components is due to some other substance than iron. The subject will, of course be investigated.

FOREIGN OBSERVATIONS.—It is yet too early to know much about the success of the foreign observations. I have, however, added in the summary* a statement of such reports as have come to hand. In Europe the weather was mostly bad, and nothing was got either at Greenwich or Paris. At the German Astro-physical Observatory, at Potsdam, the success was better, and we learn that numerous photographs were made, as well as spectroscopic observations. The most important stations, however, for combination with our own, are those in the southern hemisphere. We have already by telegraph most gratifying announcements of success from the Cape of Good Hope, from New Zealand, Tasmania, and Australia, and especially from the parties at Santiago, Chili.

The most important parties yet to be heard from are those in the neighborhood of the Straits of Magellan. There are German, French, Brazilian, and American observers in that region, and the news from them will be awaited with great anxiety. Whatever it may be, however, enough is already secured to make it certain that we have observations sufficient in number and character to test the full value of the transit as a means of determining the solar parallax.

How long it will be before the observations, (especially the photograph and heliometer measures) are fully reduced and published, it is impossible to say. It must be years at least. After this is done, it will be extremely probable that some high authority, perhaps international commission should collect and discuss all the various observations both of this transit and that of 1874, and, from the enormous mass of material thus obtained, deduce the best final result which it can furnish, a result which cannot fail to be of the highest value in settling the dimensions of our universe. *(See article preceding this.) C. A. Y.

THE SECOND COMET OF 1874 —ENCKE, as is well known, considered the alleged discovery of this comet by the Chevalier d' Angos to be a

pure fabrication; but many astronomers of the highest authority, GAUSS and D'ARREST for instance, do not agree with him in thinking that it has been proved to be an imposture. At all events the two first observations which were communicated to MESSIER may have been really made. M. BURCKHARDT, assuming these observations to be genuine and the distance of the comet from the earth to be unchanged between the times at which they were made, deduced an orbit bearing a considerable resemblance to that of the three following comets,—1743 I., 1819 IV., and Dennings' comet of last year. M. HUGO GYLDEN, not confining himself to any supposition as to the distance of the comet on the two occasions of observation, has computed a number of orbits with hypothetical values for the inclination and the longitude of the node, and concludes that the two observations *might* belong to a comet moving in an orbit not unlike that of these three comets. But the elements always gave the distance from the earth as small, so that large perturbations might be produced by that body. Following up this consideration, M. GYLDEN finds a slightly greater accord than before with the orbits of the three comets mentioned above, and concludes that if the two observations were really made, they may *possibly* refer to a comet belonging to the same group.

COUNTERPOISES OF A MERIDIAN INSTRUMENT.

The question of how much weight to leave on the Ys of a meridian instrument, is one which naturally does not admit of a definite answer. As a matter of fact the problem to be solved is to leave *enough* on the Ys to be sure that the pivots remain always in their bearers. Any more than this simply wears pivots and Ys uselessly and makes the telescope harder to move. I find that CARRINGTON left fifteen pounds on each Y, at Red Hill; KAYSER at Leiden lift about 15.4 pounds, GOULD at Cordoba has two kilograms, 4.4 pounds. When the Repsold meridian circle arrived here about ten pounds was the pressure on each Y, but the two Ys bore unequal loads. I have made them equal and each about 11.9 pounds. In this shape a weight of one pound applied either at eye or object-end will just keep the telescope moving.

To find the weight on each Y a very simple method was suggested to me by one of my students — Mr. CONRADSON. He proposed to place a stiff rod of the proper length vertically under the axis close to the Y. The upper end of this supports the axis. The lower end rests in the scale-pan of a common scale, and the pressure on the Ys can then be weighed as closely as desired. I found a common spring balance firmly suspended would accomplish all I wished to do.

E. S. H.

NOTE ON TUTTLE'S COMET. (1858 I).

The aphelion of TUTTLE'S comet is just beyond the orbit of *Saturn*.

The former, therefore, in all probability, owes the present form of its orbit to the disturbing influence of the latter. The two bodies were near together about 1783, and consequently the comet's orbit must have been considerably changed at that epoch. Again fifteen periods of the comet are approximately equal to seven periods of *Saturn*, so that extraordinary perturbations of the former must occur a second time about 1988—9.

D. K.

OCCULTATION OF 4 GEMINORUM BY JUPITER.

I had the good fortune to witness the occultation of the star 4 Geminorum by the planet *Jupiter* on the night of November 7, 1882. The sky was perfectly clear and the definition excellent except at short intervals when there were slight tremors. One of these occurred just at the moment of immersion. I used the eleven-inch equatorial with an eye-piece magnifying 450 diameters. The details of the surface of *Jupiter* were brought out better than I had ever seen them before. Six oval white spots were visible in the southern hemisphere below the white belt. An indistinct dark spot near the equator passed the meridian at about 14^h 10^m mean time. The shadow of satellite I was then just beginning transit. The following are my notes on the occultation:

"Watch 12^h 2^m $10^h \pm 2^s$. Star disappeared quite suddenly. Definition was not very good so that it disappeared momentarily several times before this. I waited a couple of seconds before looking at my watch to be sure that it would not appear again. Watch then gave 13^s. Looked back immediately but the star was certainly gone. It disappeared at the lower (northern) edge of the bright red band in the upper (southern) hemisphere, as it appeared in the telescope with my eyes parallel. Power 450."

My watch was 10^s slow on Mt. Lookout mean time so that the true time of immersion was Mt. Lookout mean time, 12^h 2^m $20^s \pm 2^s$, Washington mean time 12^h 31^m $49^s \pm 2^s$.

The phenomenon was similar to an occultation by the moon in all respects, excepting the size and slowness of motion of the occulting body. I did not detect any diminuation of the star's light. It seemed just as bright when it last disappeared as when at a distance from the planet.

I did not observe the emersion. H. C. W.

The observatory at Marietta, O., is under the direction of Professor W. C. GURLEY. The telescope by Mr. BYRNE of New York is a refractor of $6\frac{1}{4}$ inches aperture, and 99 inches focal length. The circles are divided on silver, and read by vernier respectively, in right ascension to four seconds of time, and in declination to single minutes of arc. The mounting is so complete that the telescope may be operated

entirely from the eye-end. The driving-clock is a Bond Spring Governor made by the HOWARDS of Boston. The range of eye-pieces is from 65 to 652, and are seven in number with solar and star diagonals and an amplifier conveniently mounted. The instrument is to be supplied with double-image and filar micrometers Professor YOUNG of Princeton has made drawings for a solar spectroscope, to be supplied with the Rutherford grating, or a prism, at pleasure. When all these accessories are in hand, Professor GURLEY will be ready for useful work in astronomy in many ways He speaks of the definition of his instrument as "simply perfect."

At Carleton College observatory systematic mapping of sun-spots was commenced Oct. 13 by W. E. CATHCART, assistant, according to the Carrington method. Up to, and including Nov. 20, observations were made on 23 days, and every time spots were seen. Sixteen groups were recorded during this period, two of which were probably the return of those previously seen. One of the two groups at its first appearance included 50 distinct spots as shown with the equatorial of $8\frac{1}{4}$ inches aperture. Between Oct. 20 and Oct. 23 the leading spot disappeared. Before the group appeared the second time, all the spots had united. From Nov. 9 to Nov. 20, faculæ were numerous and unusually bright. The nights of the 12, 17, and 19, were remarkable for brilliant auroras.

Professor E. FRISBY wishes to make a correction in one of the elements of the great comet of 1882 published last month. The value given to *phi* was 89° 7′ 42″.70. It should be *phi* = 89° 13′ 42″.70.

The copy received for his observation of the third contact of the transit of *Venus* was wrong. It read $2^h\ 38^m\ 57^s$. It should read $2^h\ 39^m\ 57^s$.

It is interesting to learn that the ephemeris computed from the elements of the comet above referred to agree with those found by Mr. FINLAY at the Cape of Good Hope within 7 seconds of arc in right ascension, and 1″.5 in declination.

By kindness of Professor PURINTON we have the following report from the observatory of Furman University, at Greenville, S. C.

My telescopic observations upon the transit of *Venus* were very satisfactory, giving me an opportunity to view all four contacts with great accuracy. Owing to a misunderstanding with the telegraph operator, I failed to get Washington time and so make no report of local time of contacts knowing that I cannot do so without error. The sky was cloudless and atmosphere unusually clear during the whole transit. Phenomena due to the atmosphere of *Venus* were very marked and enclu-

sive. Saw nothing like a 'black-drop.' Used Alvan Clark & Son's refractor, aperture five inches. Had intended to have photographs, but the artist disappointed me a day before the transit. What I had hoped to make a success in connection with my observations proved in some measure a total failure fron circumstances beyond my control.

G. D. P.

In a recent letter, J. D. DEVOR of Elkhart, Ind., spoke of the formation of an astronomical society at that place. An effort is now being made to raise funds to purchase a telescope of $6\frac{1}{2}$ inches aperture.

The following important circular under date of Jan. 10, 1883 from Professor SPENCER F. BAIRD, secretary of the Smithsonian Institution, Washington, D. C. explains itself:

Arrangements having been completed with the Director of the Harvard College Observatory for conducting the system of telegraphic announcements of astronomical discoveries, which was established by this Institution in 1873, you are hereby informed that from and after this date, the American center of reception and distribution of such announcements will be "The Harvard College Observatory, Cambridge, Massachusetts," to which address all astronomical telegrams should in future be sent. It is hoped and believed that this transfer of a highly important service will prove beneficial to the interests of astronomical science.

Professor C. L. DOOLITTLE, director of Sayre observatory of Lehigh University at South Bethlehem, Pa., sent report of transit observations which came too late for last issue. It follows:

First contact not well observed owing to clouds and haze.

Second contact, Dec. 5, $21^h\ 22^m\ 17.^s4$;—sunlight first seen all around the planet. At $21^h\ 22^m\ 34.^s3$ light around the planet persistent and continuous. The time first recorded seemed to me to be the true time of geometrical contact. Although there was considerable haze and the air was quite unsteady the observation seemed very satisfactory. Third contact, $2^h\ 46^m\ 28^s.4$, thread of light between disc of *Venus* and limb of the sun first broken. At $2^h\ 46^m\ 28^s.2$, discontinuity of thread light persistent Image somewhat unsteady but the observation seemed very good.

Fourth contact $3^h\ 6^m\ 8^s.9$, notch in sun's limb seemed to vanish. At $3^h\ 6^m\ 30.^s9$ contact undoubtedly passed. This contact was much less satisfactory than either the second or third on account of the irregular and unsteady image of the limb. *Venus* was visible after the contact as a narrow thread of light, but was not seen before the first contact. No 'black-drop' was seen. All contacts were truly geometrical and time was local mean time. Latitude $40°\ 36'\ 23.''9$. Longitude $6^m\ 40.^s19$ east of Washington.

C. L. D.

WM. H. NUMSEN of Baltimore, Md. favors us with his observations of the transit of *Venus*.

		h	m	s
First contact, power, 120			lost.	
Second " " 200		9	16	25
Third " " 200		obscured by clouds.		
Fourth " " 200		2	59	41

Times by chronometer by Negus, noted by an assistant, corrected for error and reduced to Washington mean time, comparisons being made on the Washington noon signal, Dec. 4, 5, 6, and 7. The telescope was a 4-inch equatorial by T. COOK & SONS of York, England, solar diagonal used with colored sun shade.

Second contact uncertain by five or ten seconds. Definition bad; time noted when a brightening was first seen, true sunlight seen later. The ring of light around *Venus* was seen when following limb was yet on dark sky, it was somewhat broader at middle point.

Fourth contact, definition, better; feel sure that notch was off when time was called, and hardly think I can be more than five seconds late.

When clouds cleared up from sun after third contact, *Venus* was too far out for the arc of light to be visible. No white spot seen upon planet in transit although carefully looked for, nor was the 'black-drop' noticed at second contact. When fully upon sun the planet was not fully black, but light grey patches of light could be seen, somewhat of a dappled appearance.

Clouds covered the sky at intervals, but fourth contact was perfectly free.

W. H. N.

Mr. J. D. ELLIOTT of St. Louis, Mo. has a Byrne equatorial of 3⅜ inches aperture. In a recent letter he speaks of Mr. BYRNE'S work very favorably.

Professor GOODWIN D SWEZEY of Crete, Nebraska, says that Doane college of that place has secured a transit instrument from Messrs. BUFF & BERGER, Boston, costing $500. It is constructed for work in engineering and astronomy.

HENRY W. FAUST of San Francisco, Cal. has recently ordered a reflecting telescope of Mr. JOHN A. BRASHEAR of Pittsburg.

Want of space only has prevented us from giving the leading aspects of the planets, from month to month, as is customary in such periodicals; and we are aware that some have regretted the omission of this matter. If possible, such facts and phenomena will be given in the near future.

The index of vol. 1, of this publication will be bound with the March number.

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